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SIAMESE TURTLES

(SEE PAGE 243)

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VOLUME 29 JULY, 1938 NUMBER 7

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Vol. XXIX JULY, 1938 No. 7

CONTENTS

TWINNING IN TURTLES
By Samuel F. Hildebrand 243

RESEARCH INSTITUTES FOR BIOLOGY
By Frederick S. Hammett 255

A SAUCER PEACH BUD VARIATION
By A. D. Shamel 259

TOWARDS CURING DIFFERENTIAL BIRTHS AND LOWERING TAXES—PART II
By Caroline H. Robinson 260

COLOR INHERITANCE IN THE MEADOW-VOLE
By Frank H. Clark 265

AIDS TO THE IDENTIFICATION OF MONOZYGOTIC TWINS IN CATTLE
By Gert Bonnier and Sven Skarman 269

Reviews and Notes

A "BERGEY" FOR THE MEDICAL MYCOLOGIST (Review of DODGE, *Medical Mycology*) 266

FOURTH INTERNATIONAL CONGRESS ON ANIMAL BREEDING 267

SEX-HORMONES IN PLANTS (Rev.) 272

OCCURRENCE OF LOP EAR 272

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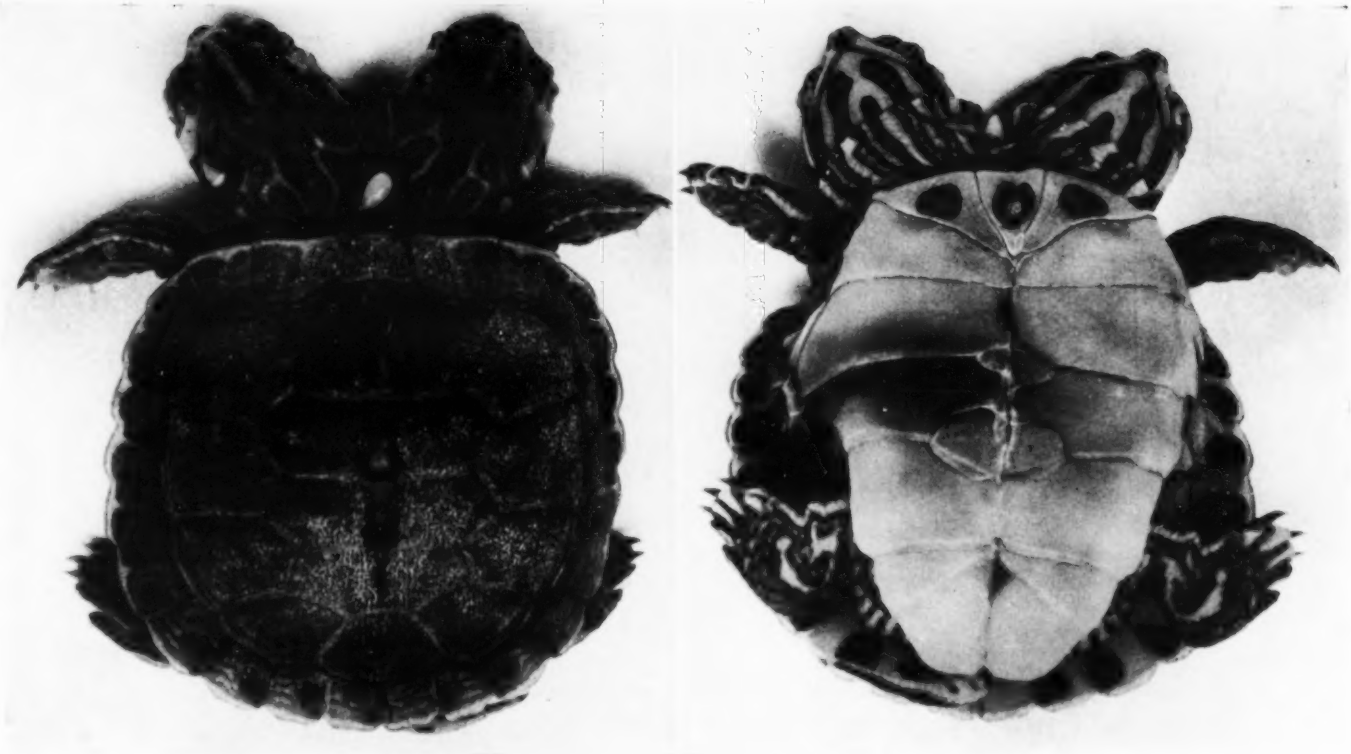

THE MOST COMMON TYPE OF DUPLEX TURTLE

Frontispiece

Two views of a two-headed yellow-bellied terrapin (*Chrysemys scaber*). This type of partial twinning is the most common encountered. In this case only the head was double, with the spinal column branched anterior to the shoulder girdle.

TWINNING IN TURTLES*

SAMUEL F. HILDEBRAND
Bureau of Fisheries, Washington, D. C.

TWINNING† very probably occurs more or less frequently among all vertebrates and among many (although apparently not all) invertebrates. It is a phenomenon which is of much general interest, and it has been a puzzle to students for ages. Although much headway has been made during recent years toward solving the mystery, the origin of two or more complete individuals or double monsters from a single germ is not yet fully understood.

Twinning in turtles is rare enough to be of special interest. A search of the literature, which is not claimed to be exhaustive, has yielded records of only 21 conjoined twins, and accounts of six additional ones now are added. "Identical" twins ordinarily would be difficult to recognize after they once have hatched. Only two pairs are definitely reported, both from unhatched eggs, and a third one taken at hatching is described in this paper.

That conjoined twins are rare, at least, in diamond-back terrapins is evident from the fact that only two such monsters have appeared among about 100,000 terrapins hatched during the course of 25 years at the United States Fisheries Biological Station, Beaufort, N. C. Identical twins, as already indicated, would be difficult to recognize after hatching, and only one pair has been found, although it is quite possible that others may have escaped notice. Other abnormalities, too, have been comparatively rare among diamond-backs hatched in captivity.‡

Hamlett[5] strongly maintains that the habitual twinning in the armadillos, as well as in certain human stocks, is a phenomenon entirely separate and distinct from that phase of twinning (accidental) in which conjoined twins result, though he does admit that in the last mentioned form of twinning two complete and separate individuals sometimes may be produced. A discussion of the theories pertaining to the cause of twinning is given in the general closing remarks of this paper.

Twinning apparently occurs in all vertebrates, as already stated, yet it is by no means equally common among all classes or groups. Twinning in man is not very common, yet it is said to occur more or less regularly in certain human stocks.[5] It is common enough for nearly every community to have at least one pair of "identical twins." More than a few human conjoined, or "Siamese twins" are reported in the literature. Among the domestic animals, such as the horse and cow, twinning apparently is more unusual than in man. In certain species of armadillo, on the other hand, twinning occurs regularly. The nine-banded Texas armadillo (*Dasypus novemcinctus*) almost constantly produces four offspring, all of the same sex and all strikingly alike, that is, "identical twins." The South American armadillo (*Dasypus hybridus*) regularly produces a larger but a less constant number, that is, seven to twelve, identical young which all originate from a single ovum. However, another species of South American armadillo (*Euphractus villosus*) is said to be "non-twinning."

Duplexity is not uncommon in bird embryos, although such eggs seldom

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†Twinning as used in this article is restricted to the origin of two or more complete individuals or double monsters from a single germ.

‡For a general discussion of abnormalities in artificially hatched diamond-back terrapins see Hildebrand.[6]

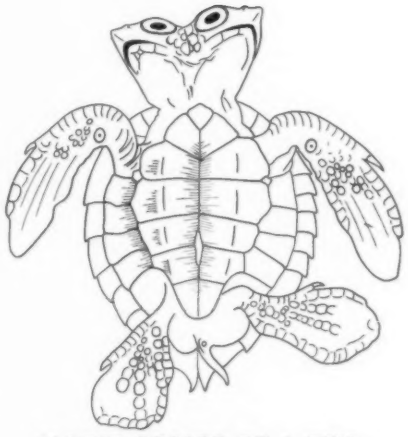
hatch. Double fish embryos are not infrequently seen in hatcheries. While the eggs usually hatch, the resulting monsters, with few exceptions, do not live long or gain much growth. A considerable number of snake monsters, usually double anteriorly, are reported in the literature. Some of them are of such a size that they must have lived for some time after hatching.

Two-headed Turtles

The earliest record of a double chelonid monster known to the present writer is found in "A Natural History of Birds," by George Edwards, published in 1751. The animal was a sea turtle and the author, although quite uncertain, thought that it was a "hawk-bill." The place of capture is not given and no description is offered. However, a figure was published which shows that duplexity had not advanced very far. The animal had two heads supported either from a common neck, or more probably from two united necks. The illustration, reproduced herewith (Figure 1), does not indicate duplexity in the body, and the plastron appears to be normal.

In 1826, Samuel L. Mitchell, in a paper in which he endeavored to show that two-headed snakes were not individuals of a distinctive race but universally monsters, incidentally mentions that he had heard of a "two-headed tortoise that lived to acquire a considerable size by taking food at both mouths."

The next account of a double-headed turtle appeared in 1888. This article, written by E. H. Barbour, contains a rather full description of the external anatomy, together with an account of the behavior, of a two-headed eastern painted turtle, *Chrysemys picta*, found in the marshes of West River, New Haven, Conn. The New Haven monster probably had been hatched only two or three days when found and fourteen weeks later it had "increased in size at least one-third." It had two normal heads of equal size, two entirely free necks and an abnormally broad carapace with some extra plates and a slightly curved vertebral line. The plastron was abnormal only in the presence of two pairs of gular plates. In other respects the external structures did not differ perceptibly from those of a normal animal.



OLDEST RECORD OF A TWO-HEADED TURTLE

Figure 1

Edwards' drawing of a two-headed sea turtle published in 1751. This creature could hardly be credited with two complete heads, as union occurs just back of the adjacent eyes.

In regard to the behavior of the New Haven two-headed turtle the author writes, "The two heads see, hear, eat, drink, sleep, breathe and move independently." The appendages of the right and left sides belonged to and responded only to their respective heads. The author said, "There seems to be absolutely no cooperation between the left side and the right, and yet they repeatedly start at the same time to do precisely the same thing, eat, swim or walk. When by any mischance it falls over on its back, the two heads work in opposite directions to right the shell, and so without help it could never, like an ordinary tortoise, extricate itself from the predicament."

The interesting discovery was made that the heads differed greatly in temperament. The author writes, "The right head, on most occasions, is the more timid and irascible, retracting or

dodging at a passing fly, or the approach of a strange animal. The left head, on the other hand, seems bold and energetic."

Between 1896 and 1933 nineteen partially twinned turtles were described in the literature. A summary of these records with complete bibliographic references, accompanied by illustrations from all of them, is made available to those who are interested through supplementary publication.*

In this literature the double-headed monsters reported are distributed through eight genera. Most of them are duplex only with respect to the heads, though several show fairly complete doubling of the anterior half of the body. In one instance a painted turtle (*Erysemys picta*) is so extensively twinned as to represent two almost complete individuals, attached at the posterior ends. This species, by the way, is represented by four examples of partial doubling. In only one other instance, that of the snapping turtle (*Chelydra serpentina*), is more than one case described in a single species. Thus there is a considerable series of gradation in the twinning process included in this turtle material. It is to be recalled again, that where complete separation of the embryos has occurred, resulting in two separate individuals, the resulting twins would not be detected except in those very rare instances in which an egg hatched or was opened under observation.

In this series, another striking instance of the independence of the two components is emphasized by the statement that the two heads will fight for a single piece of food, surely not a very cooperative attitude to be displayed by two individuals fated to live under one very small "roof"!

A Double-headed Terrapin

In March, 1933, an employee of the U. S. Fisheries Hatchery at Orangeburg, S. C., found a two-headed yellow bellied terrapin, *Chrysemys scaber*, on the dam of one of the hatchery ponds. The animal probably had just emerged from hibernation, and it evidently was hatched the previous summer, for the place of attachment of the umbicilus still was evident. This terrapin was sent to the U. S. Fisheries Aquarium in Washington where it was on exhibition until June 12, 1933, at which time it died, probably because it got too hot while it was given a "sun bath."

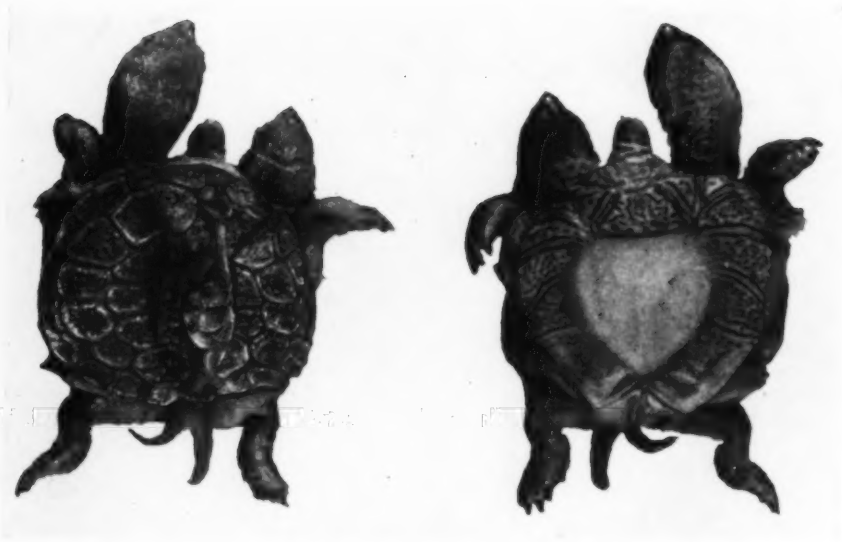
Duplexity did not proceed far in this monster. Although the heads are fully and completely developed and of equal size, the necks are joined by skin so near the heads that it must have been difficult for them to nod separately. After removing the skin and flesh, in part, it is evident that the spinal column is double only to the shoulder girdle. The carapace is abnormally broad, though no extra scutes are present. At the time of death it measured 27mm. in length and 28 mm. in width.

The plastron is quite normally developed. Slightly behind the juncture of the skin of the two necks there are two small pale fleshy protuberances, one dorsally and one ventrally, shaped more or less like Indian clubs, with the small end firmly attached to the skin. (See Frontispiece.) Since the structures are soft and fleshy and attached only to the skin, they probably are not rudimentary limbs, but abnormal skin structures.

While the animal lived in the aquarium it fed freely on lettuce, but it was not observed to take any meat. Both heads often fed at the same time, and being of equal size it was not noticed that one had an advantage over the other.

Through the courtesy of Drs. H. V. Wilson and R. E. Coker the writer has been able to examine two specimens of two-headed turtles from the collection of the University of North Carolina. Unfortunately the place and date of collection are unknown and the color is so

*Copies of this twenty-two page supplementary publication including illustrations and bibliography may be obtained by addressing the American Documentation Institute, 2101 Constitution Avenue, N. W., Washington, D. C. Request "Twinning in Turtles," Document No. 1138. The price of this publication on microfilm is 42 cents and on 4 x 6 photoprints is $2.40.



TWO HEADS AND TWO TAILS

Figure 2

Diamond-back terrapin (*Malaclemmys centrata*), in which duplexity has extended quite uniformly "fore and aft." The monster has two tails of normal size as well as two heads, and a rudimentary "fifth leg" is situated between the heads. Note that the right hand component is smaller than the left.

faded that identification is virtually impossible. The specimens probably are of the same species and they certainly belong to the pond or river turtle or terrapin group, also known as sliders and couters. The monsters presumably died, or were killed, before much if any growth had been made. One of the specimens has two heads and necks and the carapace anteriorly has a few extra shields, that is, two nuchal shields are present and between them is an exceptionally wide marginal plate. The anterior vertebral scutes are double, with a small abnormal rectangular plate between them. The plastron has three gular plates, and on the right hand side the pectoral and abdominal shields are united.

In the second specimen from the University of North Carolina duplexity has proceeded much further. In this monster two heads placed at an angle of about 45 degrees to each other, and four front feet are present, but only two hind feet, one tail and one vent are developed. The carapace of the left hand component is nearly complete, only the marginal scutes being missing to the rear right, where the carapace of this component overlaps that of the right hand one. The carapace of the right hand component as far as it is visible is equally complete. (See Figure 5.) The plastron is double anteriorly as far back as the abdominal shields, but only one pair each of femoral and anal plates is present. The two pairs of gular, humeral, and pectoral shields are nearly or quite normal, but the four abdominal plates are all malformed. (See Figure 5.)

Duplexity in this monster apparently has progressed just a little further than in the monster reported from Algiers by Caullery.[1] At least, the plastrons are more nearly double throughout. It also has progressed further in this respect than in the monster described and figured by Cunningham.[1A]

The specimen described by Cunningham was found near Cedar Falls, N. C. When examined by that writer it had partly deteriorated. Presumably for that reason nothing is said about its identity. It can only be stated here that the monster evidently was one of the pond or river turtles or terrapins. The specimen is described as having heads "almost diametrically opposed," with the "tails" in a line almost perpendicular to the two heads. According to a diagram made from an x-ray photograph the tails cross at the base and the bony structures are single in the pelvic region. The presence of two columns is strongly suggested by the two tails, though they probably fused in the pelvic region. The situation may be somewhat similar to that in the diamond-back described in the Supplementary publication (See Figure 20). Although duplexity, with respect to the spinal column seems to have progressed further than in the University of North Carolina specimen, it did not proceed nearly as far with respect to the carapaces and plastrons, as shown by Cunningham's illustrations. It is evident, therefore, that duplexity in all structures in two-headed turtles does not advance equally. Differences in the double development of the various parts of the anatomy are evident from the various illustrations published herewith.

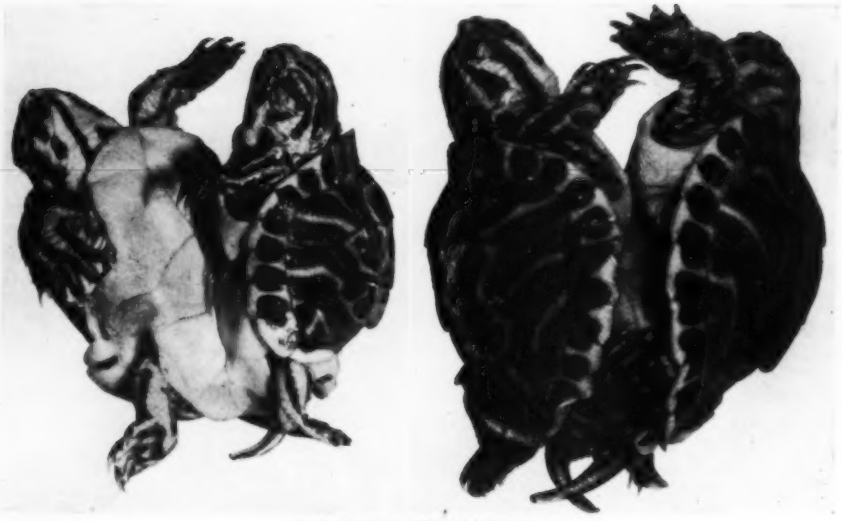
On August 17, 1935, a collector for the Florida Reptile Institute, Silver Springs, Fla., found in that vicinity among a nest of 27 terrapin eggs, in the process of hatching, three "freaks," which the present writer has been permitted to examine through the kindness of E. Ross Allen, director of the Institute. The freaks are provisionally identified as Florida terrapin, *Pseudemys floridana*. One of these terrapins has a much twisted skull and upper jaw. Only one eye is developed and its socket is nearly where the midline of the head normally would be. The upper jaw is directed much to the left and does not meet the lower jaw, which is normally developed and in normal position. In other respects, except for a rather deep furrow across the plastron just in advance of the umbilicus, the animal seems to be normally formed.

The second freak is of the usual double-headed type with the heads side by side. Duplexity progressed rather far in this terrapin. Besides two separate and normally formed heads it has four fully developed front legs and feet. Posteriorly duplexity did not advance far, however, as only one tail and two normally formed hind legs and feet are present. The presence of a single tail shows rather conclusively that the spinal columns are not double throughout. Comparing this Florida terrapin twin with the diamond-back twin, illustrated in Figure 2 it is evident that anteriorly duplexity had progressed further in the Florida terrapin and posteriorly not quite as far as in the diamond-back.

The third freak of Florida terrapin is of special interest because it is the first of its kind, so far as known to the writer, to be described. In this monstrosity two rather fully developed individuals are attached plastron to plastron posterior to the umbilici or abdominal plates only, the union being somewhat closer on one side than the other, as shown in Figure 3.

Though the two components are rather fully formed, one is somewhat smaller than the other, the smaller one having a carapace 21 mm. long, whereas the carapace of the larger one is 24 mm. long. In each component the carapace is curved upward rather sharply above the hind legs on the sides that are fartherest apart. (Figure 3, right.) The smaller one of the twins also has a deep furrow across the plastron, involving the abdominal plates, and the left hind foot of the larger one is incomplete, having only two toes and one fully developed claw. The larger component also has a fleshy wart-like protuberance between the left hind leg and the tail.

In color the two components are almost identical, though upon close examination minor differences were found, as for example the smaller one has a dark dot on the throat, separate from a single dark line behind it. In the larger one this dot is not quite separate from the



SIAMESE TERRAPINS
Figure 3

A Florida terrapin (*Pseudemys floridiana*), the first and only Siamese terrapin or turtle twins known that are attached plastron to plastron, so that the two components face each other. This monster evidently could not get its feet on the ground to walk. It survived only a short time. Note that the twins are not attached exactly symmetrically and one side is closer together than the other.

dark line behind it, which does not remain single as in the smaller one but forks posteriorly.

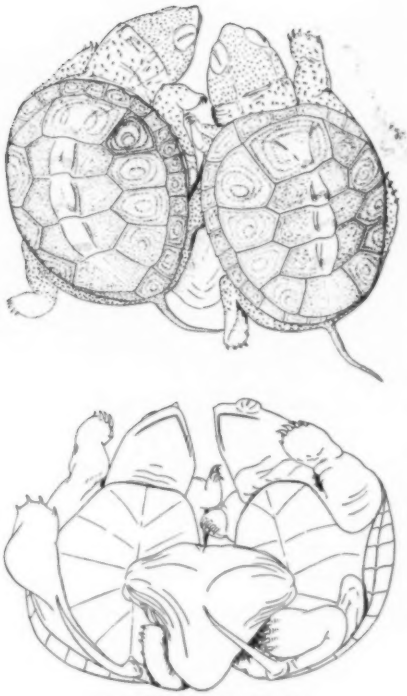
The presence of three abnormal terrapins in one nest is interesting. It has been shown experimentally, as explained elsewhere, that abnormalities may be caused among fish and starfish, at least, by chilling the eggs, or shutting off oxygen, during the early cleavage stages. Apparently anything that interferes with the normal rate of development of the early stages will result in the production of abnormal individuals. If that theory is correct then it may be assumed that some unusual condition, possibly of cold, drought, excessive moisture, or other cause interfered with the normal development of the eggs in this particular nest resulting in the production of an excessively large proportion of abnormal young.

Identical Twins

In the autumn of 1930 the first and only pair of apparently identical twins found to date among artificially hatched diamond-back terrapins was uncovered in a nest at Beaufort, N. C., by a workman engaged in digging over an egg bed toward the end of the hatching season for the purpose of removing the young. The twins were free from the egg case, yet attached to a comparatively large common yolk sac when found. Though free from the egg case, it is possible that the case was unintentionally and unknowingly broken while digging. At least, in a normal diamond-back no external yolk is present at hatching.

Although the twins are far below "normal" size (each being 18 mm. long, whereas the average length of newly hatched young is about 27 mm.), they are well formed and apparently typical diamond-backs. The shells (carapace and plastron) are moderately hard and firm and the feet have well developed claws. They were alive when found, but soon died.

The twins are typically *identical*. They



IDENTICAL TWINS

Figure 4

The only pair of identical twin diamond-back terrapins (*Malaclemmys centrata*) to be found in over a hundred thousand eggs hatched at the Beaufort Station of the U. S. Bureau of Fisheries. These twins are attached to a common yolk and are two entirely separate individuals. Separate twin turtles can only be identified by the chance discovery of such a pair just as they have emerged from the egg, or by the chance opening of an egg ready to hatch. This species usually has no yolk sac attached to the emerging infant in time of hatching, so that the egg may have been broken before natural hatching would have occurred.

are equal in size, as already stated, and of the same development throughout. In color, especially in the individual markings on the skin and shells, terrapins vary widely, and it seems impossible to find two with identical color markings. Color pattern are so nearly alike in the identical twins that even with the use of the microscope it is virtually impossible to find more than trivial differences in the individual markings such as occur, for example, on the marginal scutes. In fact, the color markings are more nearly identical than in any of the conjoined twins examined.

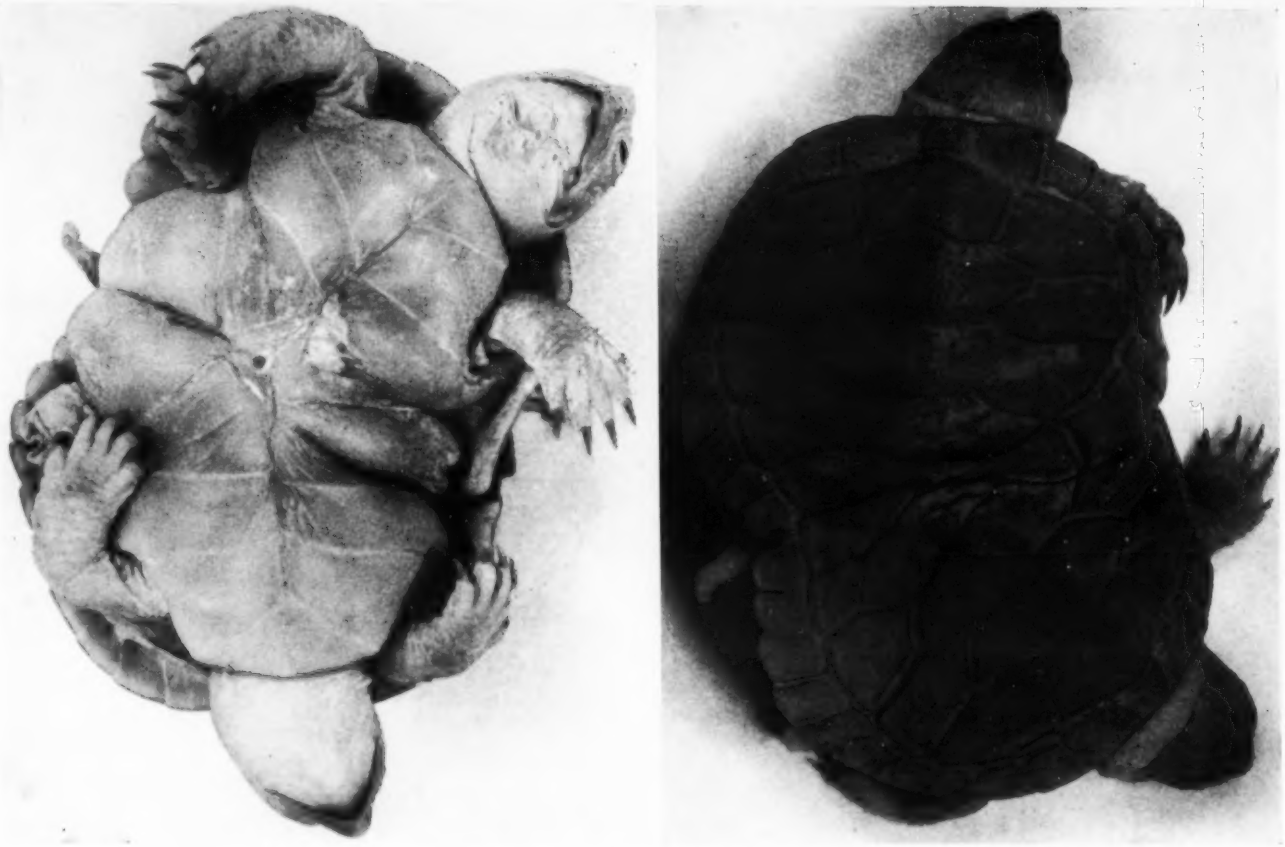
Semidiagramatic illustrations of the identical diamond-back twins, still attached to a rather large common yolk sac, are offered herewith (Figure 4). Exclusive of the early embryonic stages described by Glaesner[4] and possibly those described in the next paragraph, the diamond-back twins here described are the only identical ones known to the writer.*

P. E. P. Draniyagala[2] working with the luth or leathery turtle (*Dermochelys coriacea*) in Ceylon states that in every nest of embryos examined two eggs containing "twins" were present. However, he does not state how many nests were examined. Therefore, it is not clear whether twinning is more or less habitual with the luth.

The twins found evidentally were entirely separate, as no mention is made of conjoined twins. Under Draniyagala's discussion of "Twin Embryos" in which he deals only with mature embryos, he says, "Two nests of mature embryos were examined. Each yielded two eggs with twin embryos, while a putrid egg from a batch buried in the garden also contained twins, of which the smaller had merely a vestage of a lower jaw. The twins usually differed considerably from each other in size but in one instance the pair were almost uniform. They generally appeared at the opposite poles of the yolk and were connected by the allantois. The lesser embryo in some cases had a small yolk sac pinched off from the main yolk but always connected to the latter as well."

Two pairs of twins are rather minutely described and one pair is figured (Pl. XVI, fig. 2). Differences in the degree of scalation, in the development of the caudal crest, and in color, in addition to

*An extended review of Glaesner's report, and his figures, will be found in the supplementary publication.



SIDE BY SIDE "SIAMESE" TERRAPINS

Figure 5

A pond or river terrapin in which duplex development of the carapace (upper shell) has progressed far. Here two nearly complete individuals have evolved from the development of a single egg. Note that the "angle of development" is such that the under side (plastron) is not as completely duplex as are the carapaces.

differnces in size, are described. The differences in regard to color, scalation and the development of the caudal crest may have been merely a difference in the progress of the general development of th embryos, as the smaller one in each instance described had fewer scales, and no caudal crest or only a rudiment of one.

The writer ventures no opinion as to whether the twins originated from a common ovum, though his statement, "The lesser embryo in some cases has a small yolk sac pinched off from the main yolk, but always connected with the latter as well," seems to imply that he did not consider the eggs as double ones. It may be questioned, however, whether a partial or complete fusion of two yolks might take place during the course of development. If such a phenomenon does take place during development doubt would arise concerning the origin from a common ovum of the diamond-back twins from Beaufort. However, the diamond-back twins, as already stated, are attached to a common yolk absolutely without division, and they are so nearly identical, even to minute color markings, that their origin from a common ovum scarcely can be doubted.

Discussion

It is evident from the literature reviewed that twins have been found among a considerable number of species of chelonids, ranging from the terrestial or semiterrestial, the pond and river turtles to the marine turtles. Furthermore, twin turtles have been taken in both hemispheres and all the way from temperate to tropical regions. The majority of the twins are reported from the eastern section of the United States and most of these (13) are more or less definitely referable to the pond and river turtles. That twinning may be more common among turtles in this region than elsewhere, and that it may be more usual among the pond and river turtles than other forms is suggested, but the information available is insufficient to justify such a conclusion.

Since conjoined twins occasionally are found among turtles, "identical twins," too, would be expected. Glaesner's[4] observations on the eggs of the green turtle, *Chelonia mydas,* show rather conclusively that two separate individuals sometimes result from a single egg. This is evident also from the pair of diamond-back twins attached to a common yolk sac reported herewith (P. 248). Because identical twins very probably are not recognized when hatched, the examination of many eggs apparently would be necessary to determine how frequently one-egg twins occur among turtles.

Experimental work in the laboratory has been the chief mode of attacking the mysterious problem of twinning, and it has yielded some excellent returns. As a result light is filtering through on a dark subject. Thus, C. R. Stockard[10] found that about 23 per cent of the eggs of the common minnow, *Fundulus heteroclitus,* of the eastern shores of the United States, could be induced to form abnormal embryos by chilling them at a critical time, that is, while the eggs were passing through the early cleavage stages. Identical results were produced by allowing insufficient oxygen to reach the eggs at this sensitive stage. Chilling the eggs, as well as a deficiency in oxygen, retarded the development which evidently is very detrimental to the egg while it passes through the early cell division stages. Many twins occurred among the abnormal embryos produced by the experiments just described.

H. H. Newman[8] made the unexpected discovery that sometimes a large percentage of the eggs of the starfish, *Patiria miniata,* of the western coast of the United States developed embryos parthenogenetically, that is, without fertilization. The parthenogenetically developing eggs progressed much more slowly than fertilized ones. Then the surprising discovery was made that these slowly developing, unfertilized eggs, were producing numerous twin embryos. Newman also tells us that as many as a third or even half of the embryos, in some cases, showed some phase of twinning. The same investigator learned, also, that the development of the eggs of this starfish was less rapid when fertilized with

the sperms of another species of starfish or sea urchin than it was when the eggs were impregnated with sperm of their own species. The cross fertilized eggs, too, produced a large percentage of twins.

Newman made the further discovery that the percentage of twins could be increased by greatly crowding the starfish eggs. The crowding, presumably, caused a low oxygen and a high carbon dioxide content of the sea water in which the ova were incubated.

His conclusions, based on the experiments described, were that in each case excessive twinning resulted because the normal rate of development was retarded. That writer[9] believed, in view of his experiments with starfish eggs and other investigations and studies made by him, that most if not all developmental anomalies are the result of interference with the normal rate of development of organisms.

Stockard[10] and Newman[9] apparently regard the habitual twinning in the armadillo as an "anomally" caused by a slowing down of the development, somewhat as they produced twinning experimentally in the laboratory with fish and starfish eggs. Stockard says, "* * * the blastocysts lie free in the uterus during the 'period of quiescence.' This fact enables us to go one step further since the lack of attachment and, therefore, lack of oxygen supply are very probably 'the factors responsible for the slowing down of the developmental rhythm.'"

Hamlett[5] strongly maintains that the habitual twinning in certain species of armadillo is not an "anomaly" and not caused by a slowing down of the developmental process, as held by Stockard, and by Newman. He points out that in at least three other mammals, namely, the roe-deer, the European badger, and the American badger, there is a so-called quiescent period, during which the blastocysts lie free in the uterus, but which are not polyembryonic. He therefore holds that the cause for twinning in the armadillo must be sought elsewhere. Hamlett further makes the interesting point that double monsters are unknown in the armadillo, though they should be abundant if due to the same causes that produced twinning in the experiments reported by Stockard and by Newman. He points out, furthermore, that double monsters are not any more common in "human twinning families" than in the general population. Hamlett arrives at the conclusion, "* * * that the production of double monsters is not due to the same causes as is specific polyembrony" (as in the armadillo). "The first," he says, "is accidental, specific polyembryony is the result of a definitely inherited generic factor." He does not deny, however, that in "accidental twinning" two complete individuals sometimes are produced.

The evidence and the theory advanced by Hamlett seem plausible. Since no specific evidence has been produced indicating habitual twinning in chelonids, all twinning in turtles for the present, at least, may be considered accidental.

How slowing down the normal rate of development of an egg, or stopping it temporarily, causes twinning is an intricate question concerning which investigators have not reached a complete agreement. It is not the purpose of the present paper to enter into a discussion of the theories relative to this complex problem. Newman[9] offers the following summary of what he thinks takes place within the egg, which to the present writer appears to be a plausible explanation:

> What retardation evidently does to the egg or embryo is more or less completely to disorganize its integrational relations, or, in a word, to deaxiate it. If through retardation the polarity of the egg or embryo becomes so weakened that there are no relatively high or low metabolic regions, all parts of the embryo are left on a parity; so that, when normal developmental conditions once more appear, any part of the embryo may become the apical point and a new gradient will be established. What usually happens, however, is that enough of the original axis of polarity persists to permit just two equivalent points on the axis, at equal distances from the original apical point, to remain in such a condition that they are favored when developmental conditions return, and these become the growing points or apical ends of twin embryos. Very commonly the developmental retardation does not seriously affect the blastoderm until the process of gastrulation begins, a process of extreme delicacy, as has often been pointed out. Once fair-

ly started the cleavage process seems to be easily accomplished even under great difficulties.

It is quite possible that other turtles produce a proportionately larger number of one-egg twins than the diamond-backs, or that the diamond-backs would yield a larger percentage of twins in another locality with less stable weather conditions than exist at Beaufort, N. C., for it has been shown that twinning has occurred rarely among diamond-back terrapin at Beaufort. Newman[9], working with several species of fresh water turtles at Lake Maxincuckee, Ind., found a large percentage of abnormal embryos among some batches of eggs. Some of the abnormalities represented phases of twinning. Since incubation in the chelonian eggs presumably depends entirely upon the heat of the sun, Newman believes that the development may be markedly affected by the climatic irregularities in a temperate region. It is true that the majority of the double-headed turtles known have been reported from temperate regions. However, a few chelonian monsters are recorded from the tropics.

Twinning among chelonians in nature, then, may be due to a variety of factors. Rapid changes in temperature, already mentioned, probably constitute the most important cause of twinning in chelonian and reptilian eggs, all of which depend mainly if not entirely upon the heat of the sun for incubation. The absence of moisture or the presence of too much moisture, without necessarily a pronounced change in temperature, is known to effect the development. Furthermore, there must be times when insufficient oxygen reaches the eggs. Diamond-back terrapins, for example, lay and pack their eggs in such a way that considerable air-space remains around the eggs. However, if the sand is too dry when the eggs are laid it may not "stand up" but crumble and fill all intervening spaces. In that event the eggs probably would obtain insufficient oxygen. All these factors, and probably many others, contribute to the origin of chelonian and other reptilian twins and monstrosities.

*Aid in preparing this paper was given by many persons. The writer is very grateful to all who have given help. He wishes especially to thank Dr. E. W. Gudger of the American Museum of Natural History for his helpful suggestions, for supplying references and, last but not least, for reading and criticizing the manuscript. To Dr. Bert Cunningham of Duke University, to Prof. S. Hoffman Derickson of Lebanon Valley College, to Mr. J. A. Cederstrom of the University of Minnesota, to Mr. O. C. Van Hyning of the Florida State Museum, and to Dr. James S. Gutsell and Miss Louella E. Cable of the Bureau of Fisheries, the writer's thanks are due for furnishing information, photographs, and for assistance in the preparation of illustrations. Without the help received from the persons named and others, the paper could not have been written.

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IT has been said that every human institution is the materialized shadow of a person. Such "shadows" of brick and mortar, of apparatus and ideas and ideals, do not disappear with the setting of the sun. Some of them survive and flourish for centuries after the sun of life has set for him who first cast them across the lives of men. The great men of our race might whimsically be defined as those whose "shadows" affected the fifth generation, or the fiftieth generation to follow them more than they personally touched the lives of their contemporaries. The fact that this is so is one of the basic reasons why man has gained his present high estate in the living world. Korzybski some years ago coined the term "time binding" to describe this unique faculty whereby man is able to raise himself to higher cultural stature by standing on dead men's shoulders.

Unfortunately these same human institutions, which are one of the chief reasons why man is what he is today, may and frequently do become very effective barriers to progress. By giving continuity to human activities, institutions foster cultural growth, but the setting up of institutions and conventions releases profound reactions of conservatism which often stifle further progress. Thus man's eternal strivings toward a millenium seem always fated to be wrecked on the tragic paradox that both the means to attain this goal and the barriers which make it impossible are perhaps inseparable in the very nature of human institutions.

A vital problem in carrying on from where we are may thus be to devise operations whereby these sociological Siamese Jekyl-Hyde twins may be separated without causing the death of either of them for both conservatism and inovationism are necessary. This has been done to a certain extent in the domain of law, in the statutes of mortmain, which have increasingly limited trust holdings in perpetuity, as being contrary to the public interest. A private trust fund can only continue a limited number of years, after which time the testator loses his voice in the administratiton of the property he accumulated, for in the long run the quick must be served at the expense of the dead. Only in the field of the so-called "charitable trusts" is this legal principle denied (charity in the legal sense includes religious, educational and research institutions and corporations not conducted for profit). Charity is so sweet and its motives so far above question, that in this field the dead-hand of preconceived notions may be perpetuated indefinitely. In setting up the Rockefeller Foundation its founder is said to have remarked that "perpetuity is an awful long time," a truism too often overlooked in a changing world.

Dr. Hammett's discussion of the unfortunate way in which less formal situations, this same principle of the dead hand and of the fossilized mind combine to block a rational approach to research, raises some basically important questions in social organization. The problem of conserving the many advantages which man's time binding faculties give him, and at the same time of consciously and effectively controlling the tendency of institutionalism to lead to worship of the status quo, *to inertia, to parasitism, to accepting special privilege at its own valuation, etc., is not an easy one. We find too many positive achievements and too much of promise on the plus side of the ledger, to paraphrase the Red Queen: "Off with all these 'shadows' heads!". But Dr. Hammett shows that there is need to consider ways and means of "shadow control" in the interest of further human progress, and also in fairness to those rebels and pioneers who cast the shadows in the first place, and who, could they return, would be astonished at some of the grotesque outlines which their efforts have thrown over other lives. The means whereby this should be done, are hardly clear, however, for mere governmental supervision will not do (there are plenty of dead hands, and rumor has it even possibly a dead head or so in the Federally integrated experiment stations). Plans imposed from without do not seem very promising if we may judge by the Russian experiment. There the recent "genetic purge" seems to have left control in the hands of a group whose "ideaology" fits not experimental facts but Stalin's view of Marx. And science in countries where "the state" plays procrustes seems also to be not without its distortions. So, although we do not even have a bell to put on this cat — or shadow — there is need of an inventive approach to the job.*—EDITOR.

RESEARCH INSTITUTES FOR BIOLOGY

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RESEARCH Institutes are — or should be — integrated associations of worker, place of work, kind of work, and material worked with —whether this be in a single room with simple tools and ideas, or in a group of rooms with intricate apparatus and complicated concepts. Research Institutes for Biology are such associations where living organisms are investigated by any discipline, whether they be called Cancer Institutes or Genetic Laboratories; Institutes of Anatomy and Biology or Child Development Clinics; Nutrition Laboratories or Institutes of Pathology.

It is good that many such Institutes have been established, and it is good that the many problems therein undertaken have been approached. And the knowledge which has come therefrom is good. My interest for the moment, however, is rather in the mechanisms by which this knowledge has been attained, than in the minutiae of the knowledge itself.

It is good that perfection is unattainable because it allows chance for improvement and the exercise of the minds of men as to how such improvement can be brought about. And what I have to say and to suggest in the following paragraphs is made in no spirit of cavilling criticism or with any deprecation of effort and accomplishment; but in a full, rounded, and grateful acknowledgement of what has been done in the past, of what is being done in the present; and with complete recognition that human intelligence being what it is, and human emotions being what they are, no other than what is now in existence could have been produced. The time has possibly come, however, for a little stock taking and estimation of what might be done, if anything, to increase and consolidate scientific productivity.

I think it can safely be said that each Research Institute is the result of the enthusiasm of a single individual — of an individual interest — of an individual idea.

Because of this it has happened that there is scattered throughout the land a large number of discrete groups which —with the exception of the Government established and supervised Agricultural Experiment Stations and allied laboratories—have no common purpose save extension of the boundaries of human knowledge — have no coordination of aim or accomplishment — have no integration of purpose or usefulness.

Another result, which might have been expected from the very fact that many of these Institutes were founded on a single idea, is that in not a few the problems which were posited have either been solved or taken over by others — the proponents of the problems have either died, retired, or passed on to other interests — and the buildings and equipment which were gathered together for the solution of these problems have in all essential aspects fallen into desuetude. Much valuable apparatus is lying idle gathering the dust of inutility. What life there is in these outworn Institutes is stifled, not so much by the lack of a revivifying idea, as by the lack of nourishment for the working out of the idea. The donor of the Institute is dead, the proponent of the original problem is dead, the problem itself is essentially dead, but its cold dead weight is still present, smothering the development in that particular Institute of any life giving idea which those who are left behind may have.

My concern, however, is not with the dead, save to point out that a singly-living Institute based on a single idea

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and a single person can no more survive than can a living organism living single and alone.

The inescapable conclusion is that for an Institute to survive — and by survival I do not mean merely continuing to exist; but that in addition it should in this existence be continually usefully productive — that for an Institute to survive, it is necessary that it not only have a problem of broad base; but also that it approach this problem by every possible avenue.

It should be possible within the compass of the common and fundamental properties of living things, to attack and solve almost every problem of importance to human welfare and knowledge. This being granted it might be well to make each of these fundamental and common properties of living things the broad base for a research Institute for biology. Institutes might be established not so much on the basis of disciplines as has been the method in the past; but on the basis of a fundamental property of nature. For instance, we could have an Institute of Respiration, an Institute of Reproduction, an Institute of Nutrition, an Institute of Growth, and so on,—Institutes wherein these basic properties could be investigated as processes of all living organisms—rather than as activities restricted to single forms. In such Institutes expressions of these processes would be correlated and integrated into useful interpretations of general applications; Institutes wherein all the several disciplines are used to provide the data for the correlations and integrations.

If there were vision enough and courage enough all the existing Institutes dealing with biological research could be consolidated on one or the other of these bases. The progress which would ensue from concentration of funds and materials to greater effectiveness, and from the approach to coordinated effort is incalculable.

Deeper than the fundamental properties of common possession by living things, however, lies the essence of their expression.

Underneath the expression of living is direction and substance, the state of substance, and form.

There can be no form without substance and direction. There can be no state of substance without form and substance. There can be no substance without form and state of substance. There can be no direction without substance as its medium, form as its carrier, and state of substance as its activity determinant. The nature of living resides in these four attributes; — direction — substance — state of substance — and form.

Four Pillars of Research

The basic sciences dealing with these four then are the solid pillars on which any Institute for Biological Research should be founded.

The first of these is Genetics.

Within this science is comprised the forces, the directional guidance, the determination of what course the development of living shall take; what substance shall be used not only to express the progression, but also to compose the material thereof; what state these substances shall have for the activities of living; and into what structural configuration they shall be combined.

That Genetics has so far only provided the evidence of its potentialities and the presumed carriers thereof no more detracts from its being a necessary corner-stone of all biological research, than does the fact that we are ignorant of the nature of electricity detract from its simultaneous use as an explanation and as a tool.

Perhaps by giving Genetics its proper place as the one subject concerned with the dominating and essential attribute of continuance, the necessary more may be learned of the materials and mechanisms concerned therein.

The second foundation pillar of any Research Institute for Biology should be Chemistry.

Living substance like all other is composed of chemical compounds, groupings, and derivatives. The reaction basis of living is quite clearly founded in the chemical composition of its sub-

stance. How reasonable it is then to expect that all biological investigation should have its roots in chemistry. Surely no interpretation of any biological process is complete until the nature of the chemical participants and determinants thereof is known; assuredly no explanation can be finished until the responsible chemical reactions are set forth.

The third foundation pillar is Physics. Within the compass of this science is comprised the state of substance — and the state of substance is most important to living. Permeability, imbibition, selective absorption, adsorbtion, capillarity, motility, electrokinetics, conduction, potential difference, electrodynamic field, colloid state, viscosity, osmosis, surface tension, *H*-ion concentration, redox potential, temperature, radiant energy, and the like, are one and all decisive factors and physical factors, concerned not only in the acquisition and maintenance of state of substance in living matter, but also in the rate and extent of substance activity.

Not to have a Department of Physics as one of the four points of departure for investigation in a Research Institute for Biology is to fail to provide an organizing foundation essential for coordination.

Having established Genetics, Chemistry, and Physics as representing the three basic attributes of direction, substance, and state of substance, the fourth — that of form — must of course be presented by Anatomy.

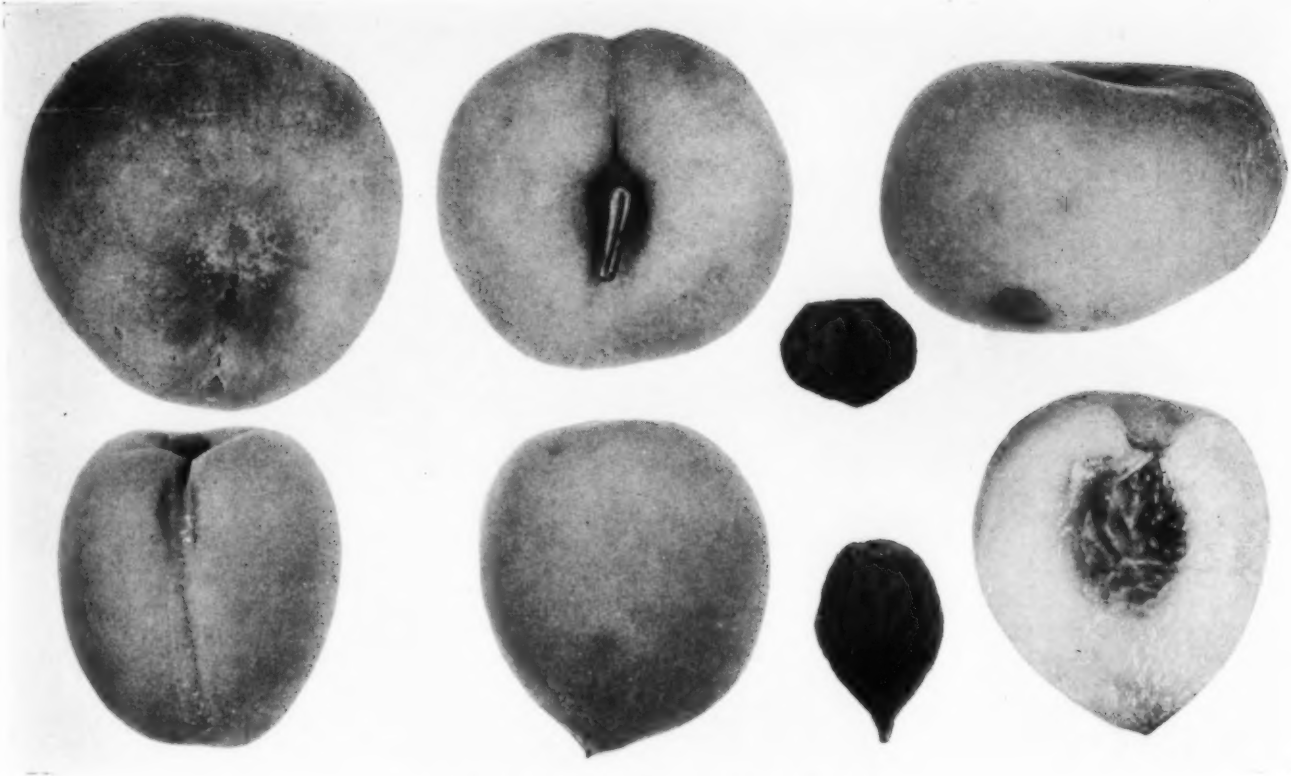
The form which substance takes in living things, the shape, the arrangement, the juxtaposition, the connections, the extensions, and the compression of substance are the elements of Anatomy. Within this science is comprised all that is concerned with architecture and structure; all that is concerned with space displacement and configuration as carriers of direction expression.

Necessary indeed as a foundation pillar to any biological Research Institute is Anatomy, for structure is at one and the same time the means of expression and the expression of function — of living.

Prolix exposition is unnecessary. The proposition is almost self-demonstrating that the simplification of biological research on the broad base of fundamental and common properties of living things, subtended by the bedrock attributes of direction — substance — state of substance — and form, would consolidate effort which is now diverse and scattered; would bring order out of what is now chaos; would provide that coordination between the several aspects of any given problem which is essential to its true solution; and would open the path for that integration from which only can come the wise incorporation of scientific findings into the daily life of man.

Secondary to simplification would be the almost immediate recession of the flood of unrelated papers; and a consequent drop in number of journals needed for publication of scientific findings. Indeed the simplification could be extended so that there need be but eight or ten journals in all. One to cover each of the general fields which would be hospitable to all work aimed at evaluation thereof; and one to cover each of the basic attributes of direction, substance, state of substance, and form.

This is really all that is necessary and sufficient. Whether or not consolidation will be attempted depends on how far human intelligence can control human emotions.



SAUCER PEACH AND "HONEY" MUTANT

Figure 6

The upper row shows typical fruits and seed of the Saucer peaches from Murphy tree. The lower row shows typical Honey peaches and seed from branch variation on the same tree. The Honey peach is a popular California variety whose origin has been a mystery. Photographed June 24, 1937.

A SAUCER PEACH BUD VARIATION

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THE writer received a letter dated June 7, 1937, from Mrs. Rosalee Murphy, 135 Damascus Avenue, Coachella, California, which reported a branch variation in an otherwise normal Saucer peach tree. On June 17, 1937, the writer visited the Murphy tree and obtained the following notes on the branch variation:

The Saucer peach tree was purchased from the Armstrong Nurseries at Ontario, California, three years ago. It is a large, fine tree and is loaded with a heavy crop of typical Saucer peaches at this time. The branch variation is located on the south side of the tree, about eight feet from the ground. It is a small fruiting branch with seven peaches on it. It has very little vegetative growth and we suggested thinning the branches around it somewhat in order to stimulate its growth, if possible. The peaches on the sport branch are of oblong-oval shape, with rather pointed tips, resembling that of Lukens Honey. The peaches on the normal branches are all of the typical Saucer shape. The contrast in the shape of the fruits of the branch variation and that of the normal Saucer peaches is very striking. No top-working or budding has been done and there is no doubt in the writer's mind but that this branch is a true bud mutation.

Samples of mature peaches from the branch variation and from the normal Saucer limbs were received from Mrs. Murphy on June 24, 1937, and a photograph of them is shown in the accompanying illustration. The normal fruits and those from the branch variation were of similar color with white flesh streaked with red around the pit, freestones, and with almost identical eating qualities excepting that the Saucer peaches tasted a little sweeter. The seeds of the normal Saucer peaches were quite different in shape, size and corrugations from those of the branch variation, as shown in the accompanying photograph.

The leaf characteristics on the normal branches on this tree were similar to those of the leaves on the branch variation having reniform glands, similar serrations, shape and size of the leaves. Until a progeny test of the sport branch is carried out, as planned, it will be impossible to compare tree characteristics such as vigor and habit of growth, branching and other characters.

Origin of Luken's Peach

One of the most popular of the Honey peach strains in the Southwest, both for rootstocks on which canning and other widely cultivated peach trees are grown and for growing in the desert sections for fresh fruits, is Lukens Honey. The introduction of this strain into California is described by the daughter of the late Comly Lukens, as follows:

In February, 1894, Percival Nichols, a florist from Bridgeton, New Jersey, brought to his friend, Comly Lukens, in Riverside, California, a Honey peach tree. He said a friend had sent this variety to him from Florida, that it had not proved satisfactory in New Jersey, but that he believed it would do well here (Riverside).

My father, Comly Lukens, formerly from Philadelphia, cared for the tree and as it thrived and bore well he budded trees from it for our own use and gave trees to neighbors. A nurseryman budding orange trees for father, having enjoyed the fruit wrote for buds. Father sent them to him and later in the *California Cultivator* we read of the Honey peach as the Lukens Honey peach.

The occurrence of the Honey-like branch variation in an otherwise normal Saucer peach tree suggests the possibility of the origin of the Honey peach from Saucer trees through bud mutation, as the characteristics of the fruits on the branch variation of the Saucer peach tree described in this note are similar in appearance to those borne by Lukens Honey trees located in Southern California.

TOWARDS CURING DIFFERENTIAL BIRTHS AND LOWERING TAXES

II. Eugenic Custody for Unfit Breeders

CAROLINE H. ROBINSON

IN the present state of politics and of religious opinion, many American states must depend upon compulsory segregation as the ultimate defense against births among the very worst problem-people. The *threat* of imprisonment, too, might be employed in a very salutory manner among the next higher grade of permanent-problem-people who are careless about having more children than they have any capacity for bringing up properly. And from the practical point of view it makes very little difference whether such carelessness represents an inherited or an acquired trait.

At the outset, students of heredity must understand this point—that our program fights births in Unfit homes not because it is in the least certain that the children would be by nature defective but because it is certain their upbringing will be defective. To tax-payers, already supporting one public parasite per solvent family, a defectively brought-up "dull normal" or even "bright" individual may be far more expensive than a well-nurtured and obedient though moronic day-laborer. A slum environment is even worse for a potentially normal child than for a feeble-minded child, and in either event society is paying handsomely for continuing conditions which can only be worsened by an exuberant lack of childlessness.

The preceding article gave data on the intolerable burdens on United States tax-payers. Our exact total has now been confirmed by Woofter and Whiting in the *Am. Statistical Jour.* of same date except that we included two million unconfined criminal parasites and they did not. Our article went on to give details of the South Dakota law which could serve as a basis for controlling the Unfit both in and out of institutions, especially if amended to give the Catholic Church control of its own people in a manner described. The present paper examines the objection that segregation of great numbers in controlled colonies is expensive and cruel.

Part-Time Colonization

Neither evil is inevitable. The expense might be greatly cut down by colonizing or segregating only one sex, the female. Damage to the race from males registered as "feeble-minded" or "permanent paupers" would be slight because marriage licenses would be forbidden them and it is presumed that women likely to bear children to them outside wedlock are mostly to be shut up. Also for the sake of economy no new buildings need be provided even for women. Abandoned construction camps and obsolescent factory plants or any empty buildings could be used. The Red Cross has had plenty of experience with thus housing the people of disaster areas. In England many prisons have been closed down in recent years due to the better preventive work of the police, etc. When this progress reaches the United States the prisons thus emptied can be used for eugenic restraint. However, safekeeping is of secondary importance in this whole business. If a few problem-women escaped now and then, it would matter little as they could easily be picked up again or else others to replace them. It would be of no consequence just *who* on any particular day was being stopped from adding to the undue birth rate of society's substratum. It would be sufficient to have a lot of colonies and keep them filled to the brim with a flow of women who need to have it impressed on them that

the government means business when it says their child-bearing must be curtailed. The present expense of imprisonment runs towards a dollar a day when effort is made to insure against jail-breaking and to far more when effort is made to implant habits of industry. Both these aims could be temporarily relaxed, for economy's sake, in a drive directed exclusively to cutting down the birth rate of our lowest stratum.

It is also possible that expenses could be completely obviated by utilizing an altered version of a certain unique Japanese factory system. There, many textile manufacturers do not allow the women workers, who have voluntarily contracted to work for them, to go outside the factory grounds for years. This is an economic success and it could be transformed into a humane success (as a form of segregation) by four alterations. The women should be protected (a) by a minimum wage, (b) by never contracting for more than a month's work at a time, (c) by allowing any manufacturer to be licensed to set up this system under government regulation so that the women might be protected by competition among the employers and might have considerable choice of where to work. Finally (d), the factories should allow the women to go home free and unrestrainedly to their families *daily at the close of work during one week out of every four*. This is compatible with a very great reduction in the birth rate, for there is one week in the month for nearly every woman when she is very unliable to conception. What week it is, should have to be determined for each woman by a specialist from the State Health Department.

Which is Most "Cruel"?

Half a dozen such government-regulated factories in each state would be no pleasant addition to the United States scene. But they might be much less unnatural and demoralizing than ordinary imprisonment for degenerate breeders, and considerably preferable to the present tax and charity burden of one dependent paid for by each household in the United States.

On this whole great question of the cruelty involved in segregating any human being, we see how it can be made very light in any eugenic colony, first by giving the one week's freedom just described and, second, by the avenue of complete release which is open to any woman who prefers to be sterilized—or (perhaps in the very near future) vaccinated semi-annually against conception. When one considers that the poverty-stricken mother of many is all too often, because of her present habit of annual pregnancy, already horribly ailing, or unhappy, or harassed, one becomes reconciled to seeing her made a ward of the state,—trained, disciplined, aided, perhaps even partly segregated.

Secondly, whatever expense is incurred may be only a substitute expense for the present enormous cost to the community when the children of a really mismanaged home are old enough to step outside it. They clog the schools. As they grow older they come to one's house and pretend to wash one's dishes or to grind one's scissors or to sweep one's cellar, and go away again, with the job half done, to tell a hard luck story at the relief office. Similarly in the business world. They rob and burn and teach other people's children to use cocaine and liquor and most of all they thieve, and at great cost to us they shuttle in and out of the criminal courts and jails, trailing often scores of convictions behind them. Similarly they shuttle through hospitals and charity offices. Crime alone has been calculated as more expensive to society than the whole public school system of the nation or any other of our activities whatsoever.[1] Thus the delinquent twentieth (?) of us cost, counting their hours of freedom and during their "day in court" and as a nuisance to the police, probably more than the one-fourth of us whom we keep at school. And it is from large families that pris-

oners come—at a rate considerably more than three times the normal expectation.[2]

Legal guardianship for all mental deficients seems the great first step to take, backed up by cheap but adequate segregation of the incorrigible among them and followed as soon as possible by extension of the system to moral deficients too.

A New Name Needed

But many eugenists are inclined to give all their faith and efforts rather to sterilization. Now, if this device is to have any really broad development, it seems to me it will have to follow the example of "neo-malthusianism" which: (1) changed its name to "birth control" and (2) found out that the Catholics were not the real enemies as much as had been thought. Similarly, it seems that sterilization must get another name, more decent, more popular, and next it may be found as in the case of birth control clinics that the Catholics appear a vivid obstruction in our eyes chiefly because the other major obstacles are so bad. (We who try to stem the flood of the Unfit are like a man rowing hard against wind and tide—we hate to have our opponents trailing a finger in the water over the side of the boat.)

Real obstacles are: that "sterilize" has a nasty sound suggestive of disease, mutilation, cutting, and withered desiccation. Some new name is needed, suggestive of nothing, or else of the fact that to make offspring very unlikely no more need be done than for the surgeon to tie a little thread around the tiny duct that brings the male (or female) germs to join the greater bulk of other sexual secretory matter. It gives more certain prevention to cut the ducts but for purposes of polite speech or in cases of religious scruples against mutilation, tying is enough to mention or to do. Perhaps "blocking" might become an acceptable substitute name for sterilization.

Doubtless modern sterilization is less of a shock to the patient and to all who know of him than were such operations as the complete castrating of male singers for the Vatican Choir, a practice now reformed away at Rome. At any rate, whether "tied," or "blocked," the real difficulties may be not only to get past the opposition of the Catholic Church*, but afterwards to select the proper problem-cases, to get their consent and that of their relatives after having laboriously made them to understand that no outward sign will ever give evidence of exactly what has been done to them and that no patient need be in the slightest degree unsexed by the procedure. The final humdrum difficulties will be to get their appointments with the surgeon, see that they are kept, and in the case of women to arrange for considerable hospital fees.

Because of all this, it seems best to begin by colonizing unadjusted women, possibly letting them out later as they are voluntarily "blocked." If the Catholics will not consent to such release from life confinement they must needs take come responsibility for the expense and cruelty entailed. The Catholic Church is an organization of age-long efficiency and it may well be that if the burden were placed squarely upon it, it could devise a regime of sequestration away from the opposite sex which would prove merciful and tolerable to problem people.

For yet other reasons, colonizing may be the practical policy to begin upon. First, because it is consistent with keeping all sterilization non-compulsory. Second, it is the order which has actually been followed in the only State, out of 28 having laws on the subject, where sterilization has been done on the grand scale—10,000 in California.[3] Third, many of the Un-

*Perhaps the Church may be more willing to acquiesce in cases where the individual has taken a vow of chastity to which the surgeon's work is merely added as an insurance to the State against broken vows.

adjusted if they have their health and habits improved during some months' preliminary training in a colony may then turn over quite a new leaf. On being "blocked" and released, a settled and more satisfactory sex life will be made more practicable to them. The friends and relatives will often now countenance marriage for them, as they did not before. Fourth, the lengthier policy, which means enough study of each individual to assign her to a proper colony, gives more assurance that the wives of the most really useless workers will be kept from reproducing, leaving the better types of unskilled workmen undisturbed to father the laborers who will be needed for the next generation. We must never forget that as soon as times are better, employers may be howling again for a larger labor force of an unskilled type. In the past as soon as the European unskilled were shut out, we summoned yet more slowly adjustable persons from Mexico and from some of our own more backward areas. And immediately following the birth rate of these areas responded by a pause in its decline. Similarly we shut out honest people who could not secure passports while many dishonest continued to sneak in without them. Population is like water —when you shut it off one place it flows the stronger in another. Let us not repeat these errors by choosing any method of reducing the birth rate just because it is easy rather than because it shuts off the very worst types. No one knows whether sterilization, or on the contrary colonization, may prove the more liable to the abuse of reaching those easy to reach rather than those of least promise.

Lists—and "Watchfulness"

It is astounding to reflect that with all the American money that is being spent on research into "The Use of 'And' in Shakespeare" and into "The Sixth Vertebrum in Plymouth Rock Hens," no one has done any local "Census of The Unfit Breeders" of a single community. It is true that studies like "The Jukes" have shown up the course from 1775 till now, of a single tribe of wretched and expensive wastrels, prostitutes and thieves (There again, we may ignore the question of genetic or environmental causation. That society has paid a goodly bill for the Jukes' activities, has never been questioned, as far as I am aware). But no study that I know of has been done from the remedial angle to inquire just how many females would have to be dealt with—and by what measures, in a given area, in the present year of Grace—in order to stop such a tribe in its tracks, reproductively speaking. And even less, has any whole community, however small, been completely examined as to all its Unfit parents. Yet the beginning of such a census exists in every community with any kind of organized charitable or penal institutions. Every charity or welfare office has a file of case sheets a yard long concerning clients with big families who have been in and out of the office for years seeking money help and help out of their scrapes.

With such a list as a starter, there should *eventually* be set up for each community a Master List of dependent and criminal men known to be heavy breeders and of women of like character. The making of such a List we propose as a second step, or an auxiliary step, to be taken as soon as State Mental Deficiency Commissions have gotten wardship over all the obviously feeble-minded women of child bearing age. Later this wardship could be extended to more and more people on the List in respect especially to their parenthood. The Master List should never name all persons in the community unlikely to raise worthily more than two children (if that many). That would be too controversial and far too long—for about 6 per cent of the population have an I.Q. under 70, that is, a mentality unlikely to be able to go beyond the sixth grade.[4] This Master List is to be a list of Those Greatly Needing to be Watched, especially those *always in difficulties who already have*

two children and appear likely to continue unless their fertility is controlled, either voluntarily or under the threat of "blocking" — or custody — or whatever the chosen solution.

Only in exceptional cases should the list be cumbered with names of unmarried men. With unchaste women's, only if of proved fecundity. With incest, on the contrary, seeing how profoundly symptomatic it is and also how prevalent in the base stratum of society — very many of the nice-looking girls one sees in a Reform School having been corrupted by their relatives—would it not be well to place on the Master List most of the families thus tainted? Also all downright imbeciles not in safe custody. Similarly the victims of all really monstrous hereditary ills.

Then at the public schools a secondary list should be kept of pupils having moron mentality. As these come to mating age the Marriage License Office should be notified so that every one applying can be checked against a Known Moron List, as is done now in South Dakota. Then the names of new-married morons could be sent, if Protestant, to a birth control clinic, if Catholic to his or her Confessor, in order that these always ill-paid morons may be urged to use some restraint to space out their children as much as possible and never to increase them beyond two—or at the very most, three. When they begin to do so, their names, having been passed on from the Marriage License Office to the Birth Registration Office, should finally, on the birth of a second child, be sent from the Birth Registration Office to the keeper of the Master List for her "watchfulness." It is presumed the registrar is a woman social worker. In Oregon there is now a legally constituted Eugenic Board for this work.

The hard-boiled reader will snort "Watchfulness! What kind of espionage by an old maid, or what else on earth short of steel bars, can control the midnight manners of the moron, the incestuous, the chronic good-for-nothing?"

But patience! It is *no use to rebel* at the difficulty. It is our duty, our necessity to meet this foe within our gates—to keep our soil free and uncrowded by slavish breeders—to reduce the tax-burden on the high school graduate and his one (?) child raised and married off. Modern charity and medicine have inaugurated a new policy of keeping alive and spending much care on problem people who in old times lived cheaply and died perhaps childless. Now we must balance the matter—balance not only the Federal and State budgets but also balance charity with some real discipline. By all means! "balanced diets" for the poor, but also balanced duties! And if they won't or can't in normal times find steady work they should be kept — not childless, but moderate in their parentage. Against the flood of babies out of unfit homes, our social workers will gladly serve as first line of defense when once the Boards who pay their salaries have made it clear that they may mobilize. To reinforce such case-work the ultimate defense will be female segregation with liberal vacations at home whenever the physician thinks conception is not very likely to occur.

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1. Norton, Marion S., Princeton, New Jersey, "Crime in Its Biological Aspect," p. 6. (Series of pamphlets prepared with much cooperation from the faculty.) September, 1936.

2. McCartney, Jas. L. The Foundations of Crime. *Birth Control Review,* Vol. III, No. 9, May, 1936. (n.s.)

3. Gosney, E. S. Pamph., Jan. 1, 1937 (Human Betterment Fdn., Southwest Bldg., Pasadena, Cal.) "Human Sterilization Today." Also another pamph. by Norton, above, speakingly illustrated—40 photographs of degenerates, etc.

4. See preceding article, this Journal.

COAT COLOR IN THE MEADOW-VOLE

Inheritance of cream coat-color in *Microtus pennsylvanicus*, with descriptions of several other color variations

By Frank H. Clark*
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THE CREAM MEADOW-VOLE

Figure 7

The cross of the normal and cream *Microtus* produces normal colored young in the first generation, and segregation approximating a 3:1 ratio in the second generation. The finding of four color variants in this species of field mouse in a rather restricted area suggests that it may be valuable for genetic studies.

THE main purpose of this paper is to describe the appearance and inheritance of a "cream" coat-color mutation in the meadow-vole (*Microtus pennsylvanicus*). Descriptions are also given of four color variations noted among specimens of mice of this species taken in Washtenaw County, Michigan, and preserved in the University of Michigan Museum of Zoology.

The "cream" meadow-vole used in this experiment were kindly shipped to the Laboratory of Vertebrate Genetics by Edwin Baker of Blue Earth, Minnesota, and were descended from two mutant individuals of this character caught by him at different times in the same field. Altogether, he has sent fourteen of these mutants, which attests to the normal fertility of the original pair.

Animals homozygous for this character are creamy white to yellow in color and have normally pigmented eyes. The hairs of white individuals as a rule have a creamy tinge of color and, in extreme cases, the hairs of the pelage are a bright yellow throughout her length. Both types of cream may appear in the same litter. The character is similar and perhaps homologous to cream in the black rat, *Rattus rattus*.

Normal and cream *Microtus* (Figure 7) when mated together produce normally colored F_1 young. Matings between F_1 individuals have thus far produced 51 normal and 12 cream (yellow or white) mice. This is a deviation of only 3.8 ± 2.32 from the ratio of 47.2

*Contribution from the Laboratory of Vertebrate Genetics, University of Michigan.

normal to 15.8 cream that would be expected if cream is a simple Mendelian unit character. A few F_1 animals, heterozygous for cream, were backcrossed to cream mutants, giving rise to 9 normals and 6 creams, only a slight deviation from the expected 1:1 backcross ratio. These numbers, although small, offer additional evidence that cream depends upon a single recessive gene for its expression.

Variations in the cream character may be due to other genes which influence the action of the one basic gene.

Five meadow-voles of the subspecies *Microtus pennsylvanicus* which differ markedly from the usual normal dark gray color have been captured at various times in Washtenaw County in the vicinity of Ann Arbor, Michigan, and have been preserved in the Museum of Zoology. The description of these color variants is as follows:

1. ♂ 76526 and ♂ 76427, adults. The first mouse was caught by a dog in back of a residence in the outskirts of Ann Arbor and brought to the museum by F. Walworth, March 19, 1936. Live traps were then set in the same locality and the second variant, ♂ 76427, was found dead in a trap by Don Hayne and the author, March 23, 1936.

The second specimen, ♂ 76427, is very similar in color to the brown agouti or cinnamon house-mouse (*Mus musculus*). The underfur of this mouse is chocolate in color and the hairs have the normal agouti banding. According to Ridgway's "Color Standards and Color Nomenclature," (1912) this animal approximates a Snuff Brown or Cinnamon Brown in general color tone.

The first specimen taken, ♂ 76426, is similar in external appearance to the second male except that it is noticeably lighter in color. The underfur of this first animal is sooty white rather than chocolate in color, while the tips of the hairs have light yellow agouti bands. Since both of these mice were caught in the same backyard, and the second one taken was without doubt a brown agouti variant, the first mouse may represent a dilute brown agouti mutation, although the dilution of the underfur is much greater than in the dilute brown house-mouse. According to the Ridgway Standards, this skin is Snuff Brown or perhaps Cinnamon Brown in color. Three live specimens trapped in the same small area were normal gray in color. They were mated in the laboratory and produced a few young, all of which are normal, and the mutant type has not been secured in the laboratory stock.

2. ♂ 67102, immature but nearly adult. Specimen trapped by S. A. Hoslett, January 7, 1933, near Ypsilanti, Michigan. The hairs of this skin are a uniform yellow color throughout their length similar to those of a yellow house-mouse. The color does not differ greatly from that of the occasional intense yellow "cream" *Microtus* mentioned above, although most of the cream mice are much lighter in color. Referring to Ridgway, we find that the general color approaches a Cinnamon Buff or Clay color.

3. ♂ 42257, adult. This specimen was brought in to the museum by Dr. E. D. Walker, December 7, 1911. This animal was a piebald and the pelage is nearly all white. There is a normal colored area on the head with a few normally colored hairs scattered among the white hairs on the neck and along the dorsal mid-line. Most of the hairs of the tail are normally pigmented.

4. ♀ 59027, adult. This female was trapped alive by Ruth Svihla and Arthur Svihla* on the outskirts of Ann Arbor, Michigan, October 30, 1927, and kept for breeding purposes but died on December 16, 1927. This mouse has been described in detail by Ruth and Arthur Svihla who state that, "Compared with Ridgway's color chart, the color of the pelage is as follows: back and sides, drab-gray at the tips of the hairs and white at the bases; belly, pale smoke-gray; nose and extending up between and slightly above the eyes, drab; dorsal streak on tail, hair brown. The eye-color was red."

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A "BERGEY" FOR THE MEDICAL MYCOLOGIST

ACCURATE diagnosis of a parasitic disease depends in large part on an accurate description of the parasite. In writing "Medical Mycology,"† C. W. Dodge has simplified the task of identifying the parasitic fungi in the same manner that Bergey and his associates simplified the task of identifying bacteria.

The weakest link in the whole fragile chain of medical practice is the technique of diagnosis. This is because accurate

*Svihla, Ruth Dowell, and Arthur Svihla. Occurrence of a mutant meadow-mouse. *Science*, n. s., May 25, 1938, Vol. 67, p. 531.

†Dodge, C. W. *Medical Mycology*. St. Louis: The C. V. Mosby Company. 1935.

diagnosis is much more difficult and time-consuming than trying a series of remedies in the hope that one of them may be effective. Non-medical workers are often surprised to learn that many patients die before their doctors have decided definitely on the nature of their afflictions. An elaborate ritual has developed so that the attending physician will not have an opportunity to change his diagnosis after the autopsy. A surprisingly large number of physicians fail all too frequently in this ultimate test. The fact that most diseased organs are hidden from view, is not a sufficient excuse for this situation. It appears to the reviewer that there may be at least three other reasons:

1. Opportunities to obtain a medical education are more rigidly limited to students with adequate financial backing than is the case in the other professions. It is very difficult to "work one's way" through a medical school. Possibly medical schools are handicapped as a consequence in competition for the bright but impecunious students.

2. The control of medical education is inevitably in the hands of successful doctors, who are able to collect enough fees so that they can afford to attend medical meetings — an activity that can hardly be financed by robbing Junior's penny bank. Granting the great amount of research sponsored by organized medicine, is the outlook of the profession that of the practical business man, rather than that of the research worker, interested in the problems more than in the profits of disease? The reviewer, while admitting so much on the *plus* side of the medical ledger, feels disturbed that such a possibility may exist.

3. The technique of diagnosis has been "mechanized." The patient submits to a large number of tests by a group of semi-skilled technicians. The physician attempts to diagnose the disease by studying these *reports*. One doctor with a large laboratory staff can handle dozens of patients simultaneously and collect numerous fees while he spends a minimum of time with each subject. But the skill and training required to enable one to isolate bacteria and fungi from lesions is more expensive and less profitable so that trained bacteriologists are found in the laboratories of city health departments rather than in those of private physicians.

For these reasons, it will probably be many decades before practicing physicians become familiar with Dodge's "Medical Mycology." But for the pathologist concerned with fungus parasites, the book is invaluable. Most of these workers have not had a formal course in systematic botany and do not understand the technique of tracing down an organism through a dichotomous key. A short chapter explaining the use of keys would probably be very helpful for those who will use the book most. One large key following this chapter containing all the genera and with page references following each genus name would facilitate use of the book. An alphabetical glossary, with an illustration accompanying each term, would assist pathologists uncertain of mycological nomenclature. These additions would make the book available to an immensely larger group than those specially trained in systematic botany and pure mycology.

From the preceding discussion it seems that this book will be used almost exclusively by non-medical mycologists tracing down unidentified species for their medical colleagues. Since most of the non-medical mycologists know little about the therapy of fungus diseases of mammals, a short chapter on this subject would interest them very much.

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IV INTERNATIONAL ANIMAL BREEDING CONGRESS

Zurich, Switzerland, August 8-11, 1939

THE first international congress on animal breeding was held in Brussels in 1910. The two following met at Scheveningen in 1923, and in Liège in 1930. The importance of such international conferences is based on the fact that in nearly all countries animal breeding and husbandry are the most essential sources of income in agriculture. The object of these

periodical meetings is to offer an opportunity of a mutual interchange of personal opinions to scientists and breeders on the more recent advances in research and practical experiences in the field of animal breeding.

On the occasion of the last meeting in Liège the unanimous wish was expressed that the next congress should be held in Switzerland. The Swiss National Exhibition and Tenth Swiss Federal Agricultural Show in Zurich in 1939 offer the inducement to Switzerland to execute the commission received in Liège. An Organizing Committee including representatives of government Authorities, of Agricultural and Animal Breeding Associations and of Animal Breeding Institutes has taken up the preparations of the 4th international animal breeding congress, and in agreement with native and foreign professional authorities has finally selected the dates from August 8th to 11th, 1939.

The congress is organized by a committee under the patronage of Swiss Federal and State authorities, and in cooperation with Swiss Agricultural and Animal Breeding associations and Animal Breeding Institutes.

Representatives of State Governments, delegates of professional Association and Institutes, and individual members will be acknowledged as members of the congress. The Membership fee for the congress will be Frs. 20.—, this covering participation in all the general activities of the congress and the receipt of the printed proceedings.

The congress comprises plenary sessions, sectional meetings, demonstrations, excursions and further arrangements according to a special programme. The discussions refer to all species of farm livestock. The topics and interests determining the discussions during the congress are summarized in the different sections herein mentioned.

The work of the congress will comprise principal and sectional reports. The principal reports will be delivered by speakers selected by the Organizing Committee.

Sept. 1, 1938, Final "Filing Date"

Members or other professional experts wishing to submit a paper should give notice of their address, the title of the paper and the particular section and topic to which they consider it to belong to the General-Secretary by the 1st of September, 1938, at the very latest. Three copies of the abstracts of the papers, written if possible in one of the four official congress languages — English, French, German and Italian — must be in the hands of the General Secretary by the 1st of January, 1939. Abstracts coming in after this date cannot be considered for publication in the Proceedings. The size of the abstracts is limited to 8 typed pages of about 1,500 words in all, with a summary of half a typed page of about 100 words. The Executive Council will decide on the admission of papers or their apportionment to other than the indicated sections.

In connection with the congress, outings and excursions will be organized, the following having been proposed: One half-day outing during the congress; A visit to the Tenth Swiss Agricultural Show; An excursion of 4 or 5 days, visiting Swiss animal breeding and alpine farming centres, following the congress.

A special programme of the congress with reference to hotel accommodation, travelling facilities, social functions, etc., will appear later and be delivered by the General Secretary.

SECTION I:
Animal breeding in general, origin of farm animals, livestock genetics.
Topic 1: New contributions to the origin and the history of farm animals.
Topic 2: Race or breed as a notion in modern livestock breeding.
Topic 3: Genetic investigations on farm animals with regard to economically desirable and undesirable characteristics.
Topic 4: Modern animal breeding methods, their biological foundation and practical application.

SECTION II:
Fertilization, Development and Growth.
Topic 1: Artificial insemination.
Topic 2: Intra-and extra-uterine development.

SECTION III:
Efficiency of farm animals and tests.
There will be six topics under this section: (1) Conformation, etc., (2) milk yield, (3) meat production, (4) working efficiency, (5) wool quality, (6) other performance and efficiency tests.

SECTION IV:
Animal nutrition.
Two topics: (1) Chemical composition, etc., of food, and (2) Nutrition.

SECTION V:
Animal husbandry and hygiene.
Topic 1: The fight against diseases.
Topic 2: Low-land and alpine pasturing.

SECTION VI:
Official measures for the advancement of animal breeding and exploitation of products.
Topic 1: Teaching and research. (2) National and international collaboration.
Topic 2: Reports on organizations for national and international collaboration on the advancement of animal breeding.
Topic 3: Questions regarding the national and international exchange of breeding stock.
Topic 4: Contributions to the methods and the accomplishment of statistical inquiries in the field of animal breeding and husbandry.
Topic 5: What measures for the advancement of animal breeding have proved to be especially successful?

For information and advising purposes it is suggested by the Organizing Committee that National Committees should be formed in the different countries. Such appointments should be communicated to the General Secretary.

For further particulars please apply to the General Secretary, Dr. W. Engeler, Zug (Switzerland).

AIDS TO THE IDENTIFICATION OF MONOZYGOTIC TWINS IN CATTLE

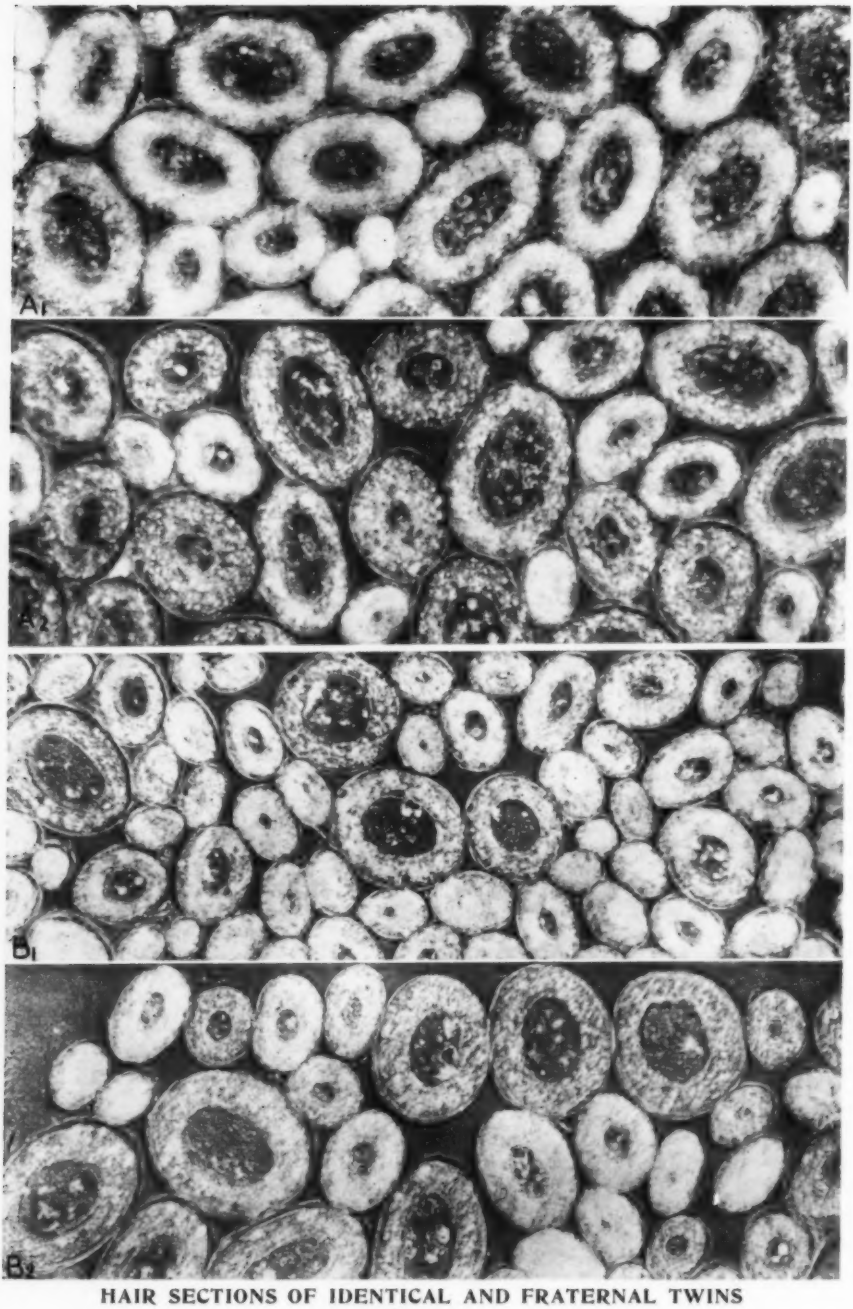
GERT BONNIER AND SVEN SKARMAN
Animal Breeding Institute, Wiad, Eldtomta, Sweden

IN late years twin research has attracted much interest; above all monozygotic twins have been the object of a large number of investigations. In regard to man the most important contribution would seem to have come from Newman and his collaborators, Freeman and Holzinger, who have recently[4] summed up their results in an excellent book. Besides comparisons between twins reared together in their homes, they also have managed to collect data for 20 pairs of monozygotic twins (one pair of which have previously been described by Muller[3]) separated at an early age, and consequently reared under more or less differing external conditions. Later, at adult age, these pairs have been subjected to physical and psychological tests of various kinds. In this way very important information of considerable reliability has been obtained regarding the great importance of environment for the moral and mental qualities of man.

Twins in Cattle Research

Comprehensive and extremely interesting investigations on cattle have been carried out by Kronacher[2] and his collaborators. In the main these researches have centered on questions connected with the identification of monozygosis in twins. But the problem of defining what is hereditarily fixed in milk production, and what is liable to influence by the environment, is of very great importance in cattle breeding, and evidently the problem can best be attacked by the aid of monozygotic twin research. For this reason the Swedish Animal Breeding Institute has arranged with a large number of herd owners to buy all heifer twin pairs born in their herds. As soon as these animals are received they are subjected to comparative analysis. Those assumed to be monozygotic—and, as a control, an equal number of undoubtedly dizygotic pairs—are kept at Wiad as test animals. In man the frequency of monozygotic twins is nearly 50 per cent of all twins of the same sex, but in cattle only about 10 per cent are derived from a single egg. On the basis of this ratio the institute expects to receive four or five pairs of monozygotic twins a year. In the course of the experiments, which were begun in the summer of 1937, one animal in the pair is fed on what might be called roughage-type feed while the other animal receives fodder of concentrate-type. In another series both animals will be fed on the same diet until their first lactation period is past; after that they are to be differently "prepared" during the dry period so that it can then be determined what influence this difference in adult nutrition has on the next lactation period.

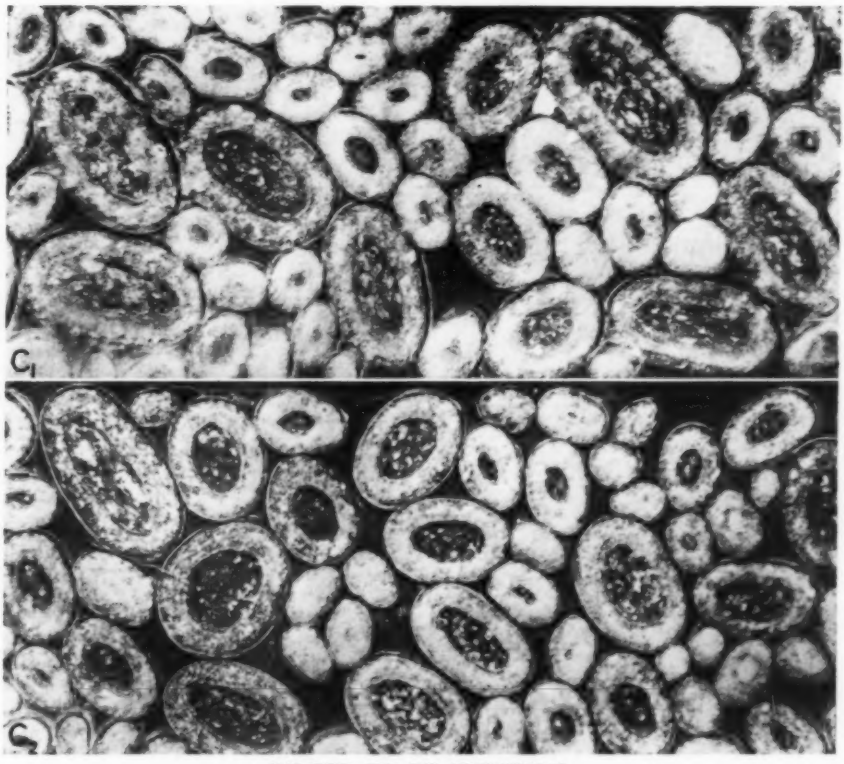
The important thing about these tests is that they are begun when the animals are only a few weeks old. Consequently it is necessary to have reliable criteria for monozygosis which can be applied before differences in feeding have a chance to produce an effect. Unfortunately such criteria are only with difficulty to be obtained. Newman and Kronacher both agree that the two twins must show striking resemblance in *many different* respects. Unfortunately for our experiment it is much more difficult to establish these resemblances in cattle than in man. Since human sibs ordinarily vary considerably in appearance, a very close general resemblance between a pair of twins of itself offers strong reason to assume that they are monozygotic. On account of the uniformity in many characteristics attained by selective breeding, the cattle comprising a breed are furnished at conception with



HAIR SECTIONS OF IDENTICAL AND FRATERNAL TWINS

Figure 8

Cross sections of hairs (250×) from the same location on the backs of two pairs of twin female calves. Each ring is a section of a single hair. Those hairs with an open space in the center are medullated; the others lack a medulla. *A*—Twin pair Beda 716-717, judged to be monozygotic (identical) on the basis of other criteria. *B*—Twin pair Emma 718-719, definitely dizygotic by other criteria. In the program undertaken at the Wiad Animal Breeding Institute to determine the extent of environmental effects through the use of identical twins, it is important to be able to differentiate monozygous and dizygous twins early in life. This criterion is proving useful for that purpose.



JUDGED TO BE IDENTICAL

Figure 9

Cross sections of hair from the twins Reginta 725-726. Here a battery of other tests agrees with the evidence of similiarity in hair cross-sections that the twins originated from a single fertilized egg.

a genetic "uniform," so that within a given breed, even though not closely related, many animals strikingly resemble each other. It follows that as large a number of identification criteria as possible must be sought for. Kronacher[2] himself suggests a number of such criteria, the most valuable of which would seem to be the mouth pattern and the shape and location of hair whorls. In addition he suggests several measurements, but some of these are uncertain and difficult to take on small calves. Nor is there any reason to believe that they will become more reliable at a more advanced age, as the calves are to be differently reared. The same applies to several of the blood characters, e.g. the hemoglobin content.

For this reason the value of various characters as a means of twin classification has been studied at Wiad. Among these characters the *hair picture*, i.e. the thickness, structure, and shape of the hairs as appearing in cross-section, seems to be particularly useful. In connection with studies of the wool characters in sheep, being carried out by one of us (Skårman), such cross-sections of hairs are measured. It therefore seemed desirable to determine whether this method might be of use in our twin re-

searches. Thus, hair samples were taken from the back of a number of twin pairs, and were prepared by Hardy's method.[1]

The result is shown in the accompanying figures. In each sample there are thicker and finer hairs, and also medullated and unmedullated hairs. If the pictures are compared in regard to appearance and proportion of the various hair types, it will immediately be found that those twin pairs which on other criteria have been assumed to be monozygotic, show close resemblance in this hair picture; while pairs which were assumed to be dizygotic differ in regard to the hair picture.

The utility of this discovery in such studies as those we are carrying on is obvious.

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Sex-Hormones in Plants

A HORMONAL THEORY OF THE DEVELOPMENT OF PLANTS. ("Gormonolnaya Teoria Razvitia Rastenii") *by* M. KH, TCHAILAKHIAN. A publication of the Institute of Physiology of Plants of the Academy of Science of U.S.S.R. Edited by K. A. Timirazev. Pp. 300, 86 illustrations. 1937.

A dissertation on plant hormones as they affect growth and reproduction. Beginning with an introductory chapter on protoperiodism and yarovization, the discussion proceeds through sex development, and the significance of leaves as growth controlling organs. The rest of the book is devoted to an exhibition of the author's view of the development of bloom-controlling hormones and of their effect in controlling the reproduction cycle in plants. The book closes with a discussion of sex hormones in plants and animals and with a final chapter on the practical utilization of methods for hastening the formation and controlling the time of flowering in horticultural practice. There is a thirteen page bibliography of Russian and German, French and English sources.—J. W. PINCUS

✦

Occurrence of Lop Ear

IN comparison with many congenital anomalies, malformations of the external ear occur infrequently and are seldom seen repeatedly in the same family tree. During the twenty year period from January 1, 1918, to January 1, 1938, 108,744 children were admitted to the Children's Hospital [Boston]. Of this number only forty-six patients were treated for malformations of the ear and of these forty-six, only twenty-one had lop ears. In interpreting these figures it must be remembered that these anomalies exist much more commonly but are of such a nature that their correction is not considered important. It is interesting to note that all but two patients with lop ears were children of Irish extraction. This series is too small to justify any definite conclusion with regard to nationalities but it is a suggestive point since the racial distribution of patients treated at the Children's Hospital is far different from the percentage that occurs in this series.—MACCALLUM, D. W., *Jour. Amer. Med. Assoc.* 110:1428, Apr. 30, 1938.

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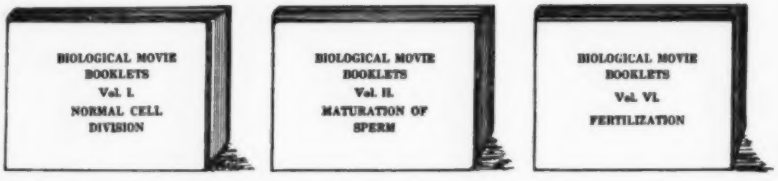

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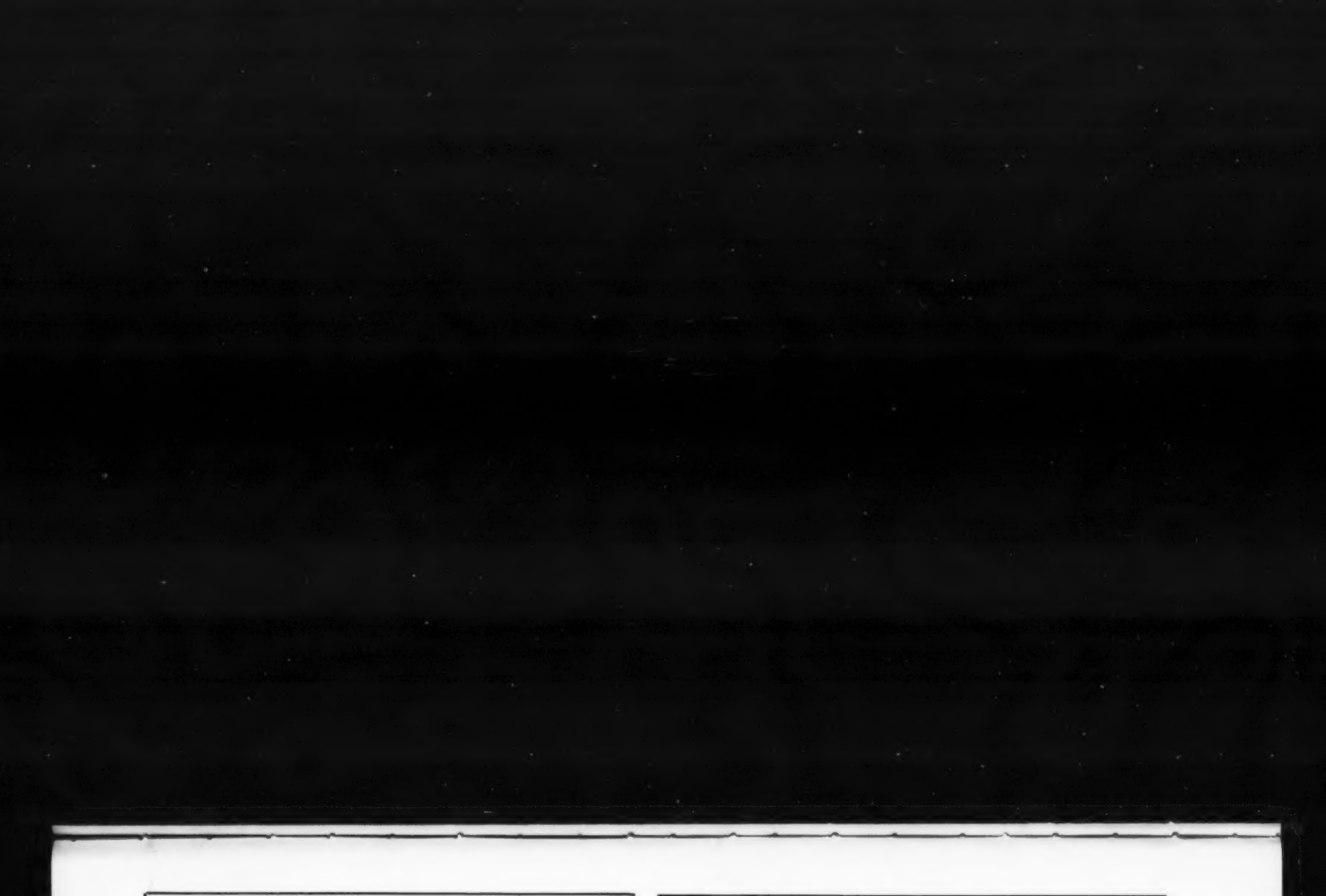
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